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## Winter's law and the issue of Balto-Slavic

If one were to judge solely by the state-of-the-art in the field of Baltic and Slavic (or Balto-Slavic; cf. below) accentology, it would sometimes seem as if genuine sound laws (which by definition must be without exception) are about as much around — that is, being discovered, formulated, and reformulated — as ever in the heyday of the neogrammarian doctrine. Suffice it to refer here to the establishment of such laws as those associated with Stang, Georgiev, Ebeling, Illič-Svityč, or Dybo, to mention just a few, which have superseded, substantially modified, or supplemented such earlier prosodic laws as those identified with de Saussure, Fortunatov, Šaxmatov, Meillet, Pedersen, Hirt, van Wijk, Dolobko, and others; cf. for some discussion Kortlandt 1977 and (now somewhat dated) Ebeling 1967. When suggesting that genuine sound laws have to be without exception — echoing the oft-cited neogrammarian dictum about the „Ausnahmslosigkeit der Lautgesetze“ — this is merely to say, of course, that for any counterexamples, or seeming exceptions, the specific factors or conditions blocking the operation of a given sound law have to be uncovered and properly defined so that such at first glance contradictory evidence can be exempt from the scope of the particular phenomenon or process covered by the applicable law. It hardly needs emphasizing that this generally accepted procedure in effect substantially weakens the force, let alone “explanatory power,” of what were once thought of as truly exceptionless sound laws, few as the latter may have been,

This is not the place to dwell in any detail on the difference between the notion of ‘law’ as opposed to ‘rule’ except to point out, perhaps, that ‘laws’ are usually thought to apply, above all, in linguistic evolution, i.e., to be tools in a diachronic approach and interpretation of linguistic change (mostly in the phonological component — hence the term ‘sound law,’ *Lautgesetz* — but in principle

also ascertainable in other parts, or on other levels, of linguistic structure). By contrast, the notion of 'rule' does not necessarily carry this narrow connotation and, in fact, at least with the advent of TG theory (and its various offshoots), is more often used as a purely notational device (occasionally wrongly, I would suggest, given more prominence than the linguistic reality it is meant to convey); in other words, 'rules' in this sense are designed to capture a generative (i.e., synchronic) process — say, the conversion from 'deep' to 'surface structure' or from syntactic ('surface') structure to phonetic implementation, and the like. Moreover, the effect of a particular rule can frequently be modified or even canceled for a certain segment of grammar, or portion of the lexico-semantic domain, by the addition of a low-level rule applicable only to that particular part of phonology, morphosyntax, or the lexicon. Thus, the "exception" to a broader rule can be accounted for by such added specific rules.

Similarly, the difference between 'law' and 'tendency,' especially in phonology, can be commented on here only very briefly. As the term suggests, a 'tendency' (or 'trend') is obviously less rigorously defined while, by the same token, frequently more comprehensive. Therefore, the problem of exceptions or counterexamples does usually not even arise here; for some further discussion, exemplified with Common Slavic data, see Raecke (1979: 193–4) who also addresses the question of the explanatory power vs. mere descriptive adequacy in the formulation of general 'tendencies' (which thus often subsume more than one single process, yet without being equatable with even more broadly conceived 'drift' in the sense used by Sapir). Common Slavic examples of typical 'tendencies' rather than strict 'laws' are, on the one hand, the tendency for palatalization (including jotation), by some believed to be reflected at one stage of the development in full-fledged syllabic synharmonism (or the phonemization of suprasegmental features, thus yielding syllabemes or group-phonemes), and, on the other, the tendency for open syllables (or syllabic structure with rising sonority); for a fairly recent discussion, see Čekman 1979: 44–175.

By and large it can be said that much, if not most, of the advances in the field of Balto-Slavic accentology have, ever since the pioneering, if controversial, work of Kuryłowicz and Stang, been concerned with studying and disclosing the correlation obtaining between accentual patterning and the shape and "curvature" of

morphological paradigms. This applies to such innovative comprehensive treatments as those by Illič-Svityč, Dybo, and Garde, among others. Yet there have also been some substantive new achievements based on close observation and thorough re-examination of the phonetic environment conditioning and triggering certain changes in the prosodic structures of Baltic and Slavic regardless of the morphological categories and patterns affected.

As is well known, accentological 'laws' are not only often applicable to accentuation proper (i.e. phenomena of stress and pitch) but they may in addition involve the third facet of prosody — quantity. This is precisely the case with what appears to be the result of a more or less regular sound shift, namely vowel lengthening, in Baltic and Slavic under specified conditions identified by the imaginative and versatile scholar and efficient research organizer whom we honor with this volume. Werner Winter published his findings in a paper first presented at the 1976 International Conference on Historical Phonology at Ustronie, Poland (Winter 1978). His contribution was immediately commented upon by Kortlandt (1978) who hailed it as a major discovery and tied it in with his own — and others' — thesis that Balto-Slavic acute intonation developed from a laryngeal feature as well as with Gamkrelidze's and Ivanov's typologically founded idea (largely paralleled by similar considerations set forth by Hoppe and Bomhard) that what traditionally has been regarded as voiced stops of PIE were in fact glottalized (or glottalic) occlusives, or rather ejectives. It was also Kortlandt who soon thereafter (Kortlandt 1977: 319–20) introduced the term Winter's Law (henceforth, WL) as a handy label for the essence of the German-American linguist's findings, at the same time restating it to amount to "the transfer of the laryngeal feature from a glottalic consonant to a preceding vowel" and suggesting "that is must be dated to the very end of the Balto-Slavic period."<sup>1</sup> In a subsequent study, Kortlandt (1979: 60–1) sought to specify the alleged two main exceptions to WL (other than, or modifying the explanation of, the seeming counterexamples adduced and discussed by Winter himself).

If in what follows I shall set forth some qualifications and reservations pertaining to WL, this is in no way meant to detract from the overall significance of Winter's findings but is merely intended to put them into what I would consider a proper perspective. As will be readily clear, part of my objections — if that is not

too strong a word — will, incidentally, be as much directed at some of the pertinent points made by Kortlandt.<sup>2</sup> Specifically, I will focus here on four issues: (1) an assessment of Winter's apparent counterexamples, including Kortlandt's evaluation of that evidence; (2) Kortlandt's claimed two exceptions to WL; (3) the significance of a comparison of the type of vowel lengthening covered by WL with subsequent instances of compensatory lengthening (henceforth, CL) in Slavic, notably in Polish; and (4) the bearing of WL on the still highly controversial question of a possible Balto-Slavic linguistic unity (or protolanguage).

Succinctly put, WL states that a PIE sequence of short vowel plus (traditional) voiced stop yields a long vowel with acute intonation plus voiced stop in Baltic and Slavic while a short vowel plus (traditional) voiced aspirate is reflected by a short vowel plus voiced stop in these languages (Winter 1978: 439). In addition to ascertaining, under specified conditions, vowel lengthening in roots of the type  $T_1eT_2$ , Winter (1978: 432) points out that vowel lengthening also occurs in bases of the type  $T_1eRT_2$ , again with  $T_2$  as a conditioning factor; however, these instances are not treated in his original paper. In support of his main thesis Winter has marshalled a numerically and in terms of systematic categorization impressive array of data, with what appears as only relatively few exceptions or counterexamples. As I have no quarrel with the vast bulk of his supportive evidence but do have some doubts regarding the counterexamples, let us therefore now reexamine these few isolated instances appearing to contradict his general conclusions.

Winter (1978: 439–43) lists merely six items, with their cognates, which at first blush appear to contradict the sound shift otherwise postulated by him. These items are: (1) Lith *pādas* 'sole, etc.', OR *podB* 'ground, bottom'; (2) Lith *sėgti* 'attach'; (3) RChSl (not OCS!) *sedB*lo 'saddle' (also OCS *osedB*lati 'to saddle'); (4) OCS *voda* 'water'; (5) OCS *bogB* 'god'; (6) OCS *xodB* 'course'. Of these etyma Winter has no satisfactory explanation for (2) and does have a fully acceptable solution for (5). Genuinely troublesome are (1), (3), (4), and (6).

As for Lith *sėgti* Winter mentions that it is frequently considered related to Slavic *\*segti* (OCS *prisešti* 'to touch'). However, semantically there is some difficulty in linking these two items; cf. Sadnik-Aitzetmüller 1955: 300, with references. At any rate, even if the Slavic word (cf. Pol *sięgać* 'to reach') is a genuine cognate, it is



nondiagnostic for our purpose since \**seg-* can reflect \**se-N-g-* as well as \**sē-N-g-*. I therefore agree with Winter (1978: 440) when he considers Lith *sėgti* a true counterexample to his rule for the time being.

Regarding *bog̃*, the view that we are dealing here with an Iranian loanword, probably replacing an earlier Slavic *diṽ* in the general meaning of 'god' (and relegating *diṽ* to other, less exalted connotations), has recently gained additional ground as shown by relevant scholarship; cf. Birnbaum-Merrill 1984: 53–4 and 58. What remains to be clarified, though, is the precise chronology of this borrowing given that Kortlandt dated the operation of WL to the end of the Balto-Slavic period (cf. below).

This then leaves us with *pādas*, *pod̃*; *sed̃blo*; *voda*; and *xod̃*. For the first set Winter proposes a new Balto-Slavic etymology, considering that the association with the IE word for 'foot', although frequently claimed, has always been tenuous, both on formal grounds (since virtually no other instances of denominal -*o* stem formations of nouns with *o*-grade root vocalism are known) and semantically (since the meanings 'foot' and 'sole' or 'ground, bottom' are said not to be particularly close). Citing Fraenkel, Winter therefore suggests that *pādas*, etc. instead be viewed as a formation with a deverbative second component derived from the IE root \**dhē* 'to put'. However, it ought to be noted that Fraenkel (1962: 92), while listing some pertinent Lithuanian and other IE items, does not count *pādas* among them and in the relevant entry (1962: 521) merely notes IE \**pēd-*/*\*pōd-* 'foot' as a cognate. It should further be remarked that the meaning of the potential verbal root in combination with the prefix *pa-* (in ablaut alternation with *pō* 'after, behind'; cf. Fraenkel 1962: 519–20 and 635) in no way appears closer to the notions 'sole' or 'ground, bottom' than the meaning of the etymon for 'foot'; for the Slavic facts, see also Vasmer 1955: 382–3, *s. v. pod*. II. Cf. further such phrases as Pol *gdzieś się ty podział?* 'where have you been (hiding)?' without any connotation of 'low' or 'deep'. Thus, while the cited Lithuanian and Slavic items do not necessarily constitute counterevidence to WL (in view of the questionable link with the lexeme for 'foot'), the proposed new etymology, too, is much too problem-ridden as to remove this word from further consideration in this context.

Where I have some more serious difficulty in following Winter is in the discussion of the three remaining items. As for the possibility

that Slavic *sedьlo* (or *sedьlo*) could be borrowed from Gothic, I consider it virtually nil. With two such outstanding specialists as Kiparsky (1975: 36, and *not* 56–7, where Gothic and Balkan Germanic loanwords are listed and discussed) and Sławski (ed. 1974: 111), I regard this word as a lexeme inherited by Slavic from IE (*Erbwort*), with the Gothic cognate exhibiting the same *e*-grade, to be sure (cf. also Feist 1939: 425, and Vasmer 1955: 601). Not only is the meaning ‘seat, chair’ in Gothic and ‘saddle’ in Slavic not that close, but Gothic *sitls*, if borrowed into Slavic, would most probably have been rendered as *\*sylь* m. (whence East and South Slavic *\*sylь*), not as *sedьlo* (or *sedьlo*) n. Absolutely nothing, except its complying with WL, would suggest a borrowing from Gothic – and that argument alone is simply not enough, least of all in an effort precisely to corroborate the validity of that sound law.

Equally unacceptable do I find the suggestion, admittedly advanced in very tentative terms, that *voda* might be a loan from Gothic as well. Winter is fully aware, of course, of the semantic problem with such a hypothesis. But what about the grammatical form and the sound shape of this word? Winter lists one ablaut cognate of *voda* in Slavic, namely *vydra* ‘otter’. In addition, however, there is also *vědro* ‘bucket’ (< *\*wēd-r-*; cf. Gmc *\*wēta-* ‘weat’; Vasmer 1953: 177) – like *vydra* showing a trace of the original heteroclitic *-r/-n* stem formation – and, with the same *o*-grade of the root as in *voda*, OR *povodь* and *povonь* ‘flooding’ (< *\*po-vod-n-ь*, thus here with *-n* suffixation; cf. Vasmer 1955: 381; R dial. also *zavod’*, *zavon’* ‘inlet, shallow water’), also noted by Kortlandt (1979: 61). Somewhat puzzling is, however, the feminine *-ā* stem formation in Slavic when compared to the IE neutral heteroclitic in *-r/-n*. Elsewhere (Birnbaum 1972: 149–51, 154, and 163) I have suggested a fairly simple – and I believe, persuasive – explanation for the origin of the Slavic feminine *-a* stem *voda* (as well as *doba* ‘time, etc.’, in Serbo-Croatian and Upper Sorbian an uninflected neuter!) from an erstwhile neuter *-r* stem.

As, finally, regards *xodь*, there are, admittedly, difficulties with this item given its initial *x-*. As is well known, Slavic *x* is the regular reflex of IE *s* only in the position after *i*, *u*, *r*, *k*, (“*ruki*-rule”). In other instances, particular reasons have been suggested. Among them are: secondary rise of an expressive or onomatopoetic *x* sound; paradigmatic leveling (especially in forms of the sigmatic aorist and in the locative plural of *-a* stem nouns); in the case of initial *x-*,

analogy with parallel compound forms with a prefix in *\*-r*, *-ī*, *(-)ũ* (this is the usual explanation for *xodъ* < *\*sod-* patterned after *\*per-*, *pri-*, *u-*, *vy-xodъ*); and, finally, borrowing from Germanic (type: *xǫdogъ* < *\*handug-*, *xlěbъ* < Goth *hlaifs*) or Iranian. The comparison with Gk ὁδός 'road' presents, again, the problem of gender switch, from feminine to masculine. However, considering that Slavic lacks feminine *-o* stems altogether, while this stem class is quite well represented in the two other genders, such a switch in an isolated item like *\*sodós* is fairly easy to conceive (change of stem class with gender retention being an alternative solution, particularly in items connoting natural gender; cf. Birnbaum 1972: 157–8 and 163). At any rate, borrowing from Iranian, not really suggested but merely considered a possibility by Winter (1978: 442), seems highly unlikely. The maximal list of Slavic items with initial *x-* possibly borrowed from Iranian compiled by Gołąb (1973: 134–52) also does not include this lexeme; cf. also Trubačev (ed.) 1981: 51–2.

Turning now to the two items on the basis of which Kortlandt thought himself able to identify the alleged two main genuine exceptions to WL, one is first of all surprised that it should be reasonable to formulate a "blocking rule" (applicable to two different, albeit comparable, phonetic environments!) on the basis of two items only. One of them is, again, *voda*, where Kortlandt obviously was not satisfied with Winter's suggestion of possible borrowing from Gothic. The other one is *ognъ* 'fire'. In both instances Kortlandt assumes the presence of two underlying clusters, each with a nasal on both sides of the voiced stop — *-ndn-* and *-ngn-* — which, he claims, precluded the operation of WL. Before entering into a discussion of the etymologies on which this assertion is based, it should be noted, though, that the sequence  $T_1eRT_2$  (where *R* represents a resonant, thus also *n*) was explicitly left out of consideration, for the time being, in Winter's own presentation of the evidence and formulation of a general rule to account for the observed vowel lengthening. At most we would have to do here, therefore, not with WL as originally stated but with an extended WL as interpreted by Kortlandt and yet to be discussed in detail by the author of the 'law'.

As concerns the crucial point with regard to the two etymologies, namely the assumption of an original (i. e., non-secondary) nasal before the voiced stop (*\*ugʷnis* said to underlie Slavic *ognъ*, Lith

*ugnīs*, Skt *agnih*, Lat *ignis*, etc.; \**vondōr* — or presumably rather \**wondōr* — as the ultimate source of both Slavic *voda* and Lith *vanduō*; cf. also Lat *unda* ‘wave’), I am uneasy about both reconstructions. For despite Hamp’s attempt to resuscitate Meillet’s old thesis with some new arguments (referred to be Kortlandt), I am still much more inclined to derive Lat *ignis* from \**egnis* and hold that Lith *ugnīs* exhibits zero-grade root vocalism (in view of Lith *agnūs* ‘fiery’); cf. Vasmer 1955: 252; Fraenkel 1965: 1158–9; Trautmann 1923: 334–5. And with Vasmer (1963: 212) and others, I would presume the nasal in Lith *vanduō* and Lat *unda* (both with further cognates showing a nasal infix, in Baltic and Latin, respectively) to be secondary rather than part of the PIE root. Consider in this connection also the general tendency for nasal infixation — primarily in the verb, to be sure — peculiar to Latin (and Italic in general) and Baltic. Be this as it may, however, I do not find it overly convincing, as previously indicated, to attempt to formulate a general “blocking rule” germane to an extended WL and covering two different, though similar environments on the basis of two questionable etymologies. Yet I would also like to reiterate that my remarks do not affect the overwhelming evidence adduced and cogently interpreted by Winter in support of the sound law bearing his name. What remains in need of further clarification, I would suggest, are merely the specifics accounting for the relatively few exceptions to it.

This said, we may now turn to the two last, broader issues: the significance of a typological comparison of WL with subsequent CL in Slavic (having its roots, it seems, in Late Common Slavic) and the claimed implication of WL as an argument in favor of the existence of a genuine Balto-Slavic linguistic unity in some distant past.

As for the former, I must state (in general concurrence with Timberlake 1983a and 1983b, and *viva voce*) that I fail fully to see the relevance of attempting to draw such a parallel. As we know, CL in Polish cannot simply be considered unqualifiedly representative of CL in Late Common Slavic or, for that matter, in the attested Slavic dialects elsewhere. Thus it was not the consonant ( $T_2$ ) as such that determined CL as this phenomenon was additionally conditioned by various sets of consonants in different Slavic regions. But the ultimate cause for CL (as, incidentally, implied by the indeed adequate term) was the loss of a vocalic element, i. e., the weak jer

after  $T_2$ , and not the secondary closure of the syllable as such. Furthermore, and not least significant in this context, in Late Common Slavic and recorded Slavic we have to do with a binary opposition of voiced vs. voiceless (or, under a different interpretation, lax vs. tense), not a threefold correlation as in PIE, which can be symbolized  $T:D:DH$ . However, if indeed  $D$  can now be reinterpreted as  $T'$  (phonetic specifics still being argued), apparently accepted also by Kortlandt, the question arises whether glottalized stops (or ejectives =  $T'$ ) before being modified may have conditioned length also in some other instances.

Finally, a few words about the relevance of WL for the unabatedly ongoing debate regarding the problem of a possible Balto-Slavic protolanguage.<sup>3</sup> Here, let us take at least a brief look at the current status of that debate. Over a decade ago, I tried to sum up the then-prevailing situation by pointing to the at least four competing (but partly compatible and thus to some degree combinable) approaches to the Balto-Slavic problem (Birnbaum 1970: 69–76): (a) the assumption of an intermediary phase of Balto-Slavic linguistic unity subsequent to PIE and preceding a separate Baltic and Slavic evolution; (b) the hypothesis of a separate, parallel evolution beginning with the disintegration of Late PIE; (c) the “modelling” approach, essentially viewing an abstract Proto-Baltic model as serving as the point of departure (or prototype) also for Common Slavic; and (d) the *Sprachbund* or, rather, convergence theory, maintaining that the many Balto-Slavic agreements are largely due to secondary contacts and symbiosis, to the extent they are not separately inherited from PIE. At the time, I indicated my own qualified preference for viewing shared exclusive Balto-Slavic characteristics primarily in the light of approaches (c) and (d); cf. similarly also Birnbaum 1967, and, for some of my subsequent thinking, Birnbaum 1973a, 1973b, and 1982: 12–13, adopting a dynamic conception of simultaneous and/or consecutive divergence and convergence. Since my overview, one of my critics (Mayer 1978: 52), an advocate of a separate development of Baltic and Slavic directly from PIE, has argued that it would have been sufficient to distinguish between only two viable approaches (namely separate, parallel vs. convergent evolution) while in a previous paper (Mayer 1977), though insisting on his theoretical premise, he in effect provided an illustration of the advantage of the “modelling” approach. More recently, another advocate of a separate develop-

ment of the two IE groups (while accepting the supplementary notion of secondary convergence; Trubačev 1982a and 1982b) has forcefully argued (1982a: 13) against conceiving of Common Slavic as a “noncontradictory model” rather than a living language. Other scholars have rehashed old arguments or introduced some new ones in support of either Balto-Slavic unity or separate development (assuming subsequent partial convergence). Among the advocates of the thesis of a Balto-Slavic protolanguage are, or were, in addition to Winter and Kortlandt, for example Vaillant (1950: 5 and 13–15, thus abandoning his teacher’s firm position against the hypothesis of such a linguistic unity), Arumaa (1964: 19–21) Georgiev (1981: 263–70, rejecting the notion of a secondary rapprochement but in some respects coming close to the “modelling” approach first expounded by Ivanov and Toporov, and dating the period of Balto-Slavic unity as early as the third millennium BC). An initial Balto-Slavic “community” (within the framework of disintegrating Late PIE) was recently advocated, with some important qualifications, by Gołab (1977 and 1983). Close to this view is also the conception set forth by Stang (1966: 13–21), while a subtly differentiated but not entirely unequivocal stand was taken more recently, in the form of a programmatic declaration setting the tone for a new serial publication, by Ivanov (1981: 6–10). On the side of those who clearly oppose the idea of any early Balto-Slavic ethnolinguistic unity are, in addition to Trubačev (cf. also Trubačev 1981) and Mayer, Filin (1980: 38–9) and Udolph (1979: 637–9, based on hydronymic data but adopting a view combining separate, parallel development with subsequent convergence and assimilation). Needless to say, the scholars just referred to represent merely a cross section of divergent opinions.

Given the wealth of the accumulated, though differently interpreted evidence — comprising anywhere between ten and twenty-five phonological and grammatical features (Arumaa 1964: 21), that is, not even counting the numerous lexical isoglosses — on which these extreme as well as some mediating opinions rest, it is difficult to see, I would think, how one more comparable or shared innovation in the phonology of prehistoric Baltic and preliterate Slavic, if indeed proven beyond any doubt, can in fact carry much weight since, in principle, this development is no different from a number of other exclusive agreements linking Baltic and Slavic as having emerged from *td* closely related dialects of IE.

## Notes

1. The realization of a causal connection between loss of laryngeal and rise of acute intonation (rising pitch) is of course not entirely new with Kortlandt but was arrived at, with various degrees of certainty and circumspection, after the definitive establishment of the previous existence of laryngeals in (P)IE; cf., e. g., Shevelov 1964/65: 46–9 and 76; Watkins 1965 (both with severe reservations, however); Vaillant 1950: 241–6. Possibly novel is Kortlandt's conception of the laryngeal 'feature' (if with the latter term is meant a suprasegmental — that is, prosodic — trait, supposedly after the loss of segmental laryngeals, i. e., laryngeal consonants). For a brief previous assessment of the Gamkrelidze-Ivanov-Hopper "new look" of PIE consonantism, see Birnbaum 1975: 13–15; 1977: 21–3. For a more up-to-date discussion, cf. Bomhard 1983: section 2.1 (the final two paragraphs).
2. I am indebted to my UCLA colleague Alan Timberlake for sharing with me his thoughts on WL. As it turned out, though arrived at independently, several of our reservations coincide. I am also grateful to him for providing me with an advance copy of his two-part study on compensatory lengthening in Slavic (Timberlake 1983a and 1983b).
3. In Winter's own words (Winter 1978: 445), "if the lengthening of vowels before reflexes of Proto-Indo-European voiced stops can be considered an established fact, one will want to insist that this phenomenon constitutes a powerful argument for postulating a Balto-Slavic unity prior to the development of separate Baltic and Slavic groups of language."

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